on the one hand, and Scrope and Lyell on the other, Etna supplied so crucial a test. Nor can we regard the few notes of Mr. Rutley on the microscopic characters of several specimens brought to him by the author, excellent as they are in themselves, as affording anything like an adequate discussion of the nature of the Etnean lavas.

There are not, indeed, wanting indications in the work before us that the author has scarcely succeeded in so far mastering the scientific questions connected with his subject as to qualify himself for giving anything like authoritative opinions concerning them. Thus on page 111 we find him speaking of a crater as "composed of a prehistoric grey labradorite, and of doleritic lava." Again, so far as can be gathered from the work before us, the hypotheses of elevation craters and eruption craters are of about equal value. We are informed simply that "the opinion of geologists is divided as to the manner in which a volcano is formed;" and then follows a statement of the two rival hypotheses. Surely after the convincing reasoning of Scrope, and the patient observations of Lyell on Etna itself, as detailed in the celebrated memoir read before the Royal Society in 1858, it is strange to find such language used upon the subject, more especially when we recollect that no attempt was ever made by Lyell's opponents to discredit his observations or to reply to his deductions. We should almost as soon expect to read in a modern work on astronomy that the opinion of astronomers is divided as to whether the earth moves round the sun or the sun round the earth.

We find so much to praise in this little book, especially in the clear *résumé* of the history of the mountain and its eruptions, and the illustrations so carefully selected and reduced from those of larger works which are not easily accessible to general readers, that we regret we cannot express more unqualified approbation of that portion of the book which calls for especial notice in the pages of this journal. We can only hope that in a second edition the author may find an opportunity, which he will not neglect, of considerably lengthening and very greatly strengthening this scientific portion of his work; and in order to do so, without at the same time impairing its popular character, we can scarcely suggest a better example for him to follow than the work of Prof. Phillips, to which we have alluded at the commencement of this article.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

The Trans-Neptunian Planet

THE explanation given by Prof. Peters (*Astr. Nach.*, 2,240) of the observations made at Washington in 1850 of this supposed planet is put beyond doubt by the examination of Mr. Ferguson's observing-book. It is due, however, to Mr. Ferguson to say that his record is full and complete, and that his changes in the reductions were honestly made. The record is in pencil, and no figures were erased or rubbed out. They are crossed out, and the assumed figure is put by the side of the original one, while at the bottom of the page is a note with pen and ink, and in Mr. Ferguson's handwriting, stating the changes that were made. Prof. Peters's ingenious discovery of the truth was made without knowledge of the observing-book.

Such criticisms are instructive, showing how unsafe it is to build theories before we are sure of the facts. They may also be a means of avoiding a waste of labour. It is known to me that at least two American astronomers, armed with powerful telescopes, have been searching quite recently for a trans-Neptunian planet. These searches have been caused by the fact that Prof. Newcomb's tables of Uranus and Neptune already begin to differ from observation. In this connection the note of Mr. Dunkin on the errors of Leverrier's tables of Saturn is interesting. But are we to infer from these errors of the planetary tables the existence of a trans-Neptunian planet? It is possible that such a planet may exist, but the probability is, I think, that the differences are caused by errors in the theories of these planets. My observations of the satellites of Saturn are not yet discussed, but they indicate that Bessel's mass of Saturn is nearly correct. Now Leverrier has diminished this mass by about $\frac{1}{125}$th, and it seems probable that this diminution was caused by some error in his theories of Jupiter and Saturn.

A few years ago the remark was frequently made that the labours of astronomers on the solar system were finished, and that henceforth they could turn their whole attention to sidereal astronomy. To-day we have the lunar theory in a very discouraging condition, and the theories of Mercury, Jupiter, Saturn, Uranus, and Neptune, all in need of revision; unless, indeed, Leverrier's theories of the last two planets shall stand the test of observation. But after all, such a condition of things is only the natural result of long and accurate series of observations which make evident the small inequalities in the motions, and bring to light the errors of theory.

Washington, March 7 ASAPH HALL

Rats and Water-Casks

MR. NICOLS says, in NATURE, vol. xix. p. 433:—

"A ship's carpenter told me that, in the old days, before the use of iron tanks on board ship became general, the rats used to attack the water-casks, cutting the stave so thin that they could suck the water through the wood without actually making a hole in it. If any one could substantiate this it would have an important bearing on the question under consideration."

Capt. Wickham, when First Lieutenant on board H.M.S. *Beagle*, told me that when he was a midshipman it was his duty, on one of the king's ships to see that certain vessels on deck were always kept full of water, in order to prevent the rats gnawing holes through the water casks, and that through such holes nearly all the water in a cask would leak away.

CHARLES DARWIN

Tides at Chepstow

I OBSERVE two letters in NATURE lately upon this subject. Many years ago I took some pains to ascertain the greatest known rise of tide at Chepstow, for I doubted the accuracy of the common statement that it was seventy feet and upwards. At the time I made the inquiry the large railway bridge at Chepstow to carry the South Wales Railway across the River Wye was being constructed. I was acquainted with Mr. Oakden, one of the engineers on the work, and he, with great care, took levels of the marks which had been made from time to time recording the very high tides, some of them going back many years. He found the highest of them to be some decimal (of which I have no record) above fifty feet above ordnance datum. I think this may be relied upon. It is corroborated in a paper by the present Astronomer-Royal, on "Tides and Waves," in the "Encyclopædia Metropolitana," vol. v. p. 242, paragraph 7, first edition. He says: "Thus, at the entrance of the Bristol Channel the whole rise at spring-tides is about eighteen feet, at Swansea about thirty feet, and at Chepstow about fifty feet." W. B. CLEGRAM

Saul Lodge, Gloucestershire, March 18

Migration of Birds

PROF. NEWTON in his article on Migration of Birds (NATURE, vol. xix. p. 433) has omitted one, and a very important limit to the height at which birds of passage can perform their journeys. This is *temperature*. The following table of Daniell's will show

© 1879 Nature Publishing Group

how little probability there is of migratory birds flying at great elevations, and that even in low latitudes, the temperature at altitudes exceeding four and a half miles would be prohibitory to the existence of the majority of migrants :—

Altitude.	Temp. F.	Temp. F.
′	°	°
0	+ 80	0
5,000	+ 64·4	− 18·5
10,000	+ 48·4	− 37·8
15,000	+ 31·4	− 58·8
20,000	+ 12·0	− 82·1
25,000	− 7·6	− 109·1
30,000	− 30·7	− 140·3

Calculations by Mr. Glaisher's rule for approximate temperature (decrease of 1° F. for every 300 feet elevation) give less startling results than the above, but even then, with the thermometer marking 80° at sea level, we find that a temperature of 40° of frost must exist at five miles in height.

The advocates of the "sight theory" have rather more in their favour than Prof. Newton has conceded to them. It is not necessary that birds should fly at such heights as to literally view the land they guide their course to or by. The "loom" of land, so well known to sailors, is visible when the land itself is below the horizon ; and I do not think we are entitled to say that birds would not, equally with mariners, notice the indication. Then again the action of one flock of birds when in sight of land, might guide other more distant flocks, and these might influence birds still further off. We know how the circling downward swoop of a vulture on some discovered carrion will draw to the feast vultures from all parts of the sky. We know the power of our own vision, certainly inferior to that of many birds; and it is therefore well within the bounds of possibility that migrating birds, watchful because weary and hungry, may see and be influenced by the movements of flocks of their companions thirty to forty miles distant. A few flocks might thus bridge a wide expanse of barren ocean.

It is not necessary, however, to insist that sight alone is the guiding faculty in migration. The majority of, if not all, animals possess that marvellous "sense of direction" that has become so blunted in civilised man. Both savages and lower animals will find their way back in a "bee-line" through unknown country, to places whence they have been led by tortuous tracks. Why should not this "sense of direction" then guide birds over oceans without landmarks. The case of first migration of young birds (cuckoos and starlings) quoted by Prof. Newton, is, it must be confessed, a problem difficult to solve ; but when the journey has been once made by an individual bird in a flock I cannot see more mystery in the arrival of that flock at their destination than there is in the perfect accord between the hand and the eye of a good shot or a good billiard player.

We must all concur with Col. Donnelly in desiring further observations, with facility for publication and discussion, and I venture to hope that we shall see many more papers from Prof. Newton's pen on the subject. E. H. PRINGLE

Scientific Club, Savile Row, March 18

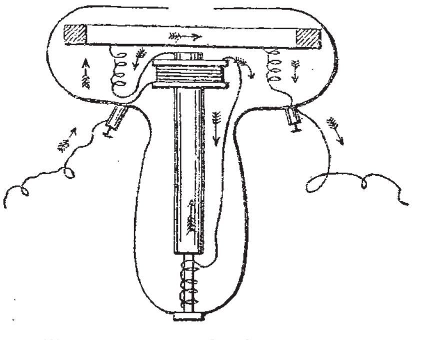
The Microtelephone

TOWARDS the end of last year I got constructed a telephonic apparatus which gives results much superior to those of the ordinary Bell telephone. Its construction is based on four principles, two of which have not yet been applied to telephones :—

1. The magneto-electric principles of the Bell telephone.
2. The microphonic principles of Hughes (different quantity of the points of contact).
3. The principle discovered by Beatson and De La Rive (1845), and which explains the experiments of Messrs. Blyth and Hughes with the speaking microphone (production of sounds by the passage alone of a discontinuous or undulating electric current).
4. The principle that the intensity of the sound depends on the density of the air in which it is produced.

All the principles are combined in so simple a manner that the microtelephone differs from the Bell telephone only in the three following points :—

1. The electric current engendered by the approach or withdrawal of the iron membrane, traversed not only the bobbin, but also the magnet and the membrane itself.
2. The communication of the current with the vibrating plate of iron is effected by means of two small springs, which are lightly pressed by the membrane, and as this pressure may be more or less strong during the action of the apparatus, the latter acts as a microphone of a relatively weak sensitiveness, but which permits the telephone to be spoken to at a distance of several centimetres, and of hearing the ticking of a watch, or the sounds of a musical box with the aid of a carbon microphone.



3. Three millimetres above the iron membrane is another membrane of caoutchouc (which should not be very fine), and both membranes inclose a layer of air, moderately compressed, which in this way must vibrate, together with the two membranes.

The microtelephone is regulated once for all, and transmits the feeblest word with a truly perfect precision.

JULIAN OCHOVOWICZ

University, Lemberg, Galicia

Vacuum Tube Phenomena

HAS it been observed that the area of the exposed surface of the negative electrode in a highly exhausted vacuum tube exerts an important influence on the facility with which the discharge takes place?

I have recently been observing Crookes's molecular shadows with a tube constructed by Mr. J. Marr, of Liverpool, in which one electrode is a flat disk about 1 inch in diameter, and the other a piece of platinum wire about ⅝ inch long. When these electrodes are connected with the terminals of an induction coil capable of giving a 4½-inch spark in air, and the contact-breaker arranged so that the shadows can just be seen when the disk is in connection with that terminal which becomes negative when the current in the primary wire is broken, a reversal of the commutator causes the discharge to cease.

If, now, the coil power be increased by the proper manipulation of the contact-breaker, a condition of things is reached in which the dark shadows flash out intermittently, even though the disk is connected with what is called the positive terminal of the induction coil.

This is evidently caused by the passage of the inverse induction current ; I mean that current which is produced when the circuit of the primary is completed. It thus appears that a condition of things can be obtained in which the effect of the greater electromotive force produced on the breaking of the primary circuit is counterbalanced by the influence of the relative size of the electrodes.

The above observation appears to be interesting, and as it may possibly be new, I venture to send you an account of it.

Nottingham, March 8 J. J. H. TEALL

Leibnitz's Mathematics

PROF. TAIT has recently given your readers one mathematician's opinion of Leibnitz as a discoverer. The following extract is serviceable in the same direction, while it has the further merit of attesting to the existence of a still later "vestige of presumption" than has yet been referred to. The extract is from a review by M. Bertrand of Dühring's "Kritische Geschichte der allgemeinen Principien der Mechanik" (Berlin, 1873). M. Bertrand says :—

Les sévérités de M. Dühring sont impartiales, et l'un des plus grands génies de l'Allemagne semble précisément le plus maltraité de tous. Les *Actes* de Leipzig, de 1684, donnèrent, est-il dit dans le texte, la première publicité à la théorie des fluxions de Newton, et en note, on ajoute : "Il n'a pas été possible d'opposer à Leibnitz des preuves complètes qui le

© 1879 Nature Publishing Group